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XXX.—*Critical Observations on Prof. Leidy's "Freshwater Rhizopods of North America," and Classification of the Rhizopods in general.* By Surgeon-Major Wallich, M.D.

From the standpoint of the evolutionist any system of classification to be strictly natural must be based exclusively on such characters as are indicative of physiological advance in the class of organisms to which it is applicable; and, *à fortiori*, every system not so based, and which, in its application, is not even coincident with readily observable physiological advance, must necessarily be looked upon as retrogressive and misleading.

With such a self-evident axiom for our guidance it will probably be admitted by every biologist who is well read-up in the scientific literature of the Rhizopods that in no class of the Protozoa has multiplication of genera and species been carried to a pitch so reckless, and certain, if left unchecked, to plunge the nomenclature of the entire class into a state of inextricable confusion.

The plea most frequently urged in justification of this mania for species-manufacture is that it is essential for the purpose of identifying particular forms. But those who rely on this plea seem to forget that identification of mere varieties does not help us in identifying types, and therefore becomes one of the most vexatious obstacles in the way of natural classification; the greater the tendency to unlimited variation

in any particular group of organisms the greater being the evil effect of ignoring these considerations. Now it is universally allowed by all who have systematically studied the testaceous Amœbans, that these organisms are, of all others, the most liable to extreme variation, in virtue of their being the most likely to be affected by external conditions and purely local influences.

Ehrenberg, the great pioneer in microscopic natural history, in touching upon this subject observed that "in the remarkable mode of reproduction by self-division and the indifference of these minute independent beings to climatic variation there appear to reside characters which sufficiently distinguish them from larger beings, so as to make them preeminently adapted to a greater duration and extension through entire and successive formation-epochs of the earth."—*Phil. Mag.* (from Trans. Roy. Acad. Berlin, 1840).

This tersely-expressed opinion has been repeatedly borrowed by later writers without due acknowledgment, and coupled with occasional additions and alterations, which have not tended to improve, but to impair, its import. In allowing myself to render it more closely applicable to the particular group of organisms forming the subject of the present inquiry, it is my earnest wish not to fall under any such imputation.

The causes affecting the stability, extension by variation, and extinction of the Protozoan species follow a law which may be thus stated:—The lower the type the less liable is it to become extinct, but the more liable is it to undergo what may be termed constructive variation, inasmuch as its simple body-substance is least powerfully affected by changes in the material condition of the medium in which it lives, whereas its protective covering (should it possess one), the basis of which is invariably chitinoid, and consists of a permanently consolidated layer of ectosarc thrown off from the animal itself, is the first portion to be acted on by extraneous conditions. We are thus enabled to explain why the body-substance of the testaceous Rhizopods remains unaltered, whereas their protective covering presents an almost infinite varietal range both as regards the materials of which it is constructed and the form the construction assumes.

With these preliminary remarks before us, let us now inquire how far the most commonly accepted subdivision of the Rhizopods into orders, viz. that proposed by Dr. W. B. Carpenter, can be considered a natural one, bearing in recollection, however, that it is to the generic and specific subdivisions of the two most thoroughly known families, namely those

furnished with shell-like or chitinoid coverings, that attention is specially invited. The question of subdivision into orders, although of primary importance as regards the basis of every system of classification, being in reality of secondary importance for the purpose now in view, is imported into it solely in order to determine the position of *Gromia*, concerning which, as will be hereafter seen, there would still appear to be a great deal of misconception.

According to Dr. Carpenter, the subdivision into orders may be best accomplished by taking as a basis "those structural characters which are most expressive of physiological difference in the *form, proportions, and general arrangement of the pseudopodial extensions*; for notwithstanding their unrestrained polymorphism, the Rhizopods present three very distinct types of pseudopodian conformation, to one or other of which they may all be referred, *the group thus formed being eminently natural.*" Dr. Carpenter then proceeds to say that "in cases in which the differentiation into ectosarc and endosarc has proceeded furthest, so that the body of the Rhizopod bears the strongest resemblance to an ordinary cell, as is the case with *Amœba* and its allies, a *nucleus* may be distinctly traced; in those, on the other hand, in which the original protoplasmic condition is most completely retained (as seems to be the case in *Gromia* and with the Foraminifera generally), no nucleus can be distinguished" *.

In Dr. Carpenter's classification *Gromia* is consequently made the type of his lowest or Reticularian order, and is associated in that order with the Foraminifera only. The same basis of classification would seem to have been adopted by Prof. Huxley in his "Hunterian Lectures on the Invertebrata," delivered in 1867, when he described the Foraminifera as a group of Monerozoa containing some of the very simplest forms of life, one of the simplest of Foraminifera being *Gromia*, a jelly-like mass with extensile pseudopodia enclosed in a horny shell, differing from the imperforate Miliolidæ and Lagenidæ only in having a membranous or horny shell †.

In the Ann. & Mag. Nat. Hist. for June 1863 it was pointed out by me that the nuclear body with its capsular investment made its appearance for the first time in the two highest orders, and not in the lowest, which in my system

* 'The Study of the Foraminifera,' 1862, pp. 14 and 15.

† "Roy. Coll. Surgeons: Hunterian Lectures by Prof. Huxley, F.R.S., on the Invertebrata." (Abstract.) Quart. Journ. Microsc. Science, 1868.

comprises the Gromidæ, Foraminifera, and Polycystina, the nuclear granules being in this order diffused, and assuming the multiple character of sarcoblasts, which, on separation from the parent sarcode, constitute the primordial segment of the new brood. It was then also stated that the contractile vesicle does not make its appearance in the lowest order, namely the Herpnemata, or the intermediate order, the Protodermata, but occurs for the first time in the highest order, or Proteina, in which are associated together the Actinophrynæ, Lagynidæ, and Amœbidæ, both nucleus and contractile vesicle being invariably present in all the families of this order, although sometimes obscured from view in the testaceous genera. At the period referred to, viz. June 1863, neither of these two organs had as yet been noticed in *Gromia*; but a few weeks afterwards the discovery of the nucleus in this Rhizopod was announced as follows:—"As bearing directly on the characters of the Amœbidæ I have to record an important fact which revealed itself during my examination of the material containing *Amœba villosa*; I allude to the detection of a well-marked nucleus and nuclear capsule in *Gromia oviformis*. The contractile vesicle I failed to trace, but, in the presence of the manifest analogy existing between the Gromidæ and Lagenidæ, it is, I think, extremely probable that this organ also may yet be detected. Should it be so, the transfer of *Gromia* from the lowest to the highest ordinal type of Rhizopod structure would be rendered necessary."—*Annals*, Aug. 1863, p. 123.

Having followed up this subject still further, the following statement was made by me in the 'Annals' for December of the same year (p. 450):—"I may here repeat the statement made in the 'Annals' for August last, p. 123, that I had detected a distinct nucleus in *Gromia oviformis*. At a later period, but only once, I detected an equally distinct contractile vesicle. But until further opportunities present themselves of determining whether or not these two organs occur universally in all the members of the genus, I would reserve my final opinion on the subject." Finally, in a paper "On the Affinities of the Polycystina," read at the Royal Microscopical Society in May, and published in Quart. Journ. Microsc. Science for July 1865, my first tabulated classification of the Rhizopods appeared, the three orders being defined as shown opposite:—

No definite nucleus. No contractile vesicle.		Definite nucleus. No contractile vesicle.		Definite nucleus. Contractile vesicle.	
1.		2.		3.	
HERPNEMATA.		PROTODERMATA.		PROTEINA.	
Shell never siliceous.	Shell invariably siliceous.	Skeleton solid.	Skeleton tubular.	Pseudopodia monomorphous.	Pseudopodia polymorphous.
Foraminifera. Lieberkuhnia (*Clap.*) ? Pamphagus (*Bail.*) ?	Polycystina.	Plagiocanthidæ. Acanthometrina. Thalassicollina.	Dictyochidæ. ↓ Spongida.	Actinophryna. *Actinophrys.* *Gromia.* *Lagynis.* *Euglypha.* *Cadium* (Bail.). *Protocystis* (Wal.). *Plagiophrys* (Clap.).	Amœbina. *Amœba.* *Difflugia.* *Arcella.* *Pseudochlamys.*

Under the head of characters relating to the Proteina it was further stated that the presence of two such organs as the nucleus and contractile vesicle must be regarded as of primary importance, reasons having already been assigned for considering the degree of differentiation of the sarcodic body alleged to be deducible from the shape, form, proportions, and arrangement of the pseudopodia as of merely secondary value; and that, after a laborious study of the freshwater Proteina extending over nearly two years, without any important intermission, I felt satisfied that, even if made the basis of generic subdivision, these pseudopodian characters "are subject to a much wider range of variation than is usually imagined, not only in the same genus, but in the same individual at different periods of its existence" *.

It was during the above-mentioned continuous study of the Proteina that I verified the fact of the presence of a contractile vesicle in *Gromia* in a sufficiently large number of cases to place the matter beyond doubt. This was mentioned in a paper "On the Fundamental Error of constituting *Gromia* the Type of Foraminiferal Structure," published in the 'Annals' for Feb. 1877, p. 168.

Meanwhile, however, Dr. Carpenter had brought out the fifth edition of his most excellent treatise on 'The Microscope,' and had so far modified his views as to insert the following remark respecting the characters upon which he still depended for the subdivision of the Rhizopods into orders:—"It must be freely admitted," he said, "that these groups [the Reticularia, Radiolaria, and Lobosa] cannot be distinctly marked out, the typical examples which will now be described being connected by many intermediate forms. This is not to be wondered at when the extreme indefiniteness which characterizes the lowest type of animal life is duly borne in mind. . . . . In *Gromia*, moreover, we have an example of a Rhizopod which very characteristically exhibits the Reticularian type in the disposition of the pseudopodia, but which Dr. Wallich was the first to point out possesses both a nucleus and contractile vesicle, *thus showing a transition to the higher orders.*"—*Op. cit.* pp. 168, 169.

It needs no argument of mine to prove that a more illogical and hazardous conclusion could not have been drawn from so very significant a fact, for, instead of the altered position of *Gromia* being in anywise accounted for by at-

* For details of the grounds on which I rested my statements concerning the worthlessness of ordinal and generic characters derived from the pseudopodia see papers on the Rhizopods in the 'Annals' for Nov. 1863 and Dec. 1868.

tributing the previously so-called typical characters of its pseudopodia to its transitionary tendency, these characters, when taken in conjunction with the vastly more important presence of the nucleus and contractile vesicle, which alone indicate the true systematic position of the organism, proved at once that the Reticularian type, as well as every other pseudopodian type, could no longer be received as indicative of physiological advance, and consequently could no longer be considered of any practical value in the subdivision into orders of the various families of Rhizopods.

Having thus shown how the case stood in the year 1877, it will now be necessary to redirect our attention to the years 1863–4, when I called attention for the first time in the 'Annals' to the occurrence in this country and elsewhere of an extensive and highly interesting series of testaceous Rhizopods which, with three exceptions to be referred to hereafter, had not previously been described and figured by any other writer. Two of these excepted forms were included in Ehrenberg's famous work 'Die Infusionsthierchen,' published in 1839, but without any observations beyond a somewhat imperfect description of their external characters, due no doubt to the inferior nature of the microscopic appliances then available. In these circumstances, and in entire ignorance of the fact just stated, I described and figured the two forms in question, together with the remainder of the really new and typical varieties of *Difflugia* which had been discovered by me in India and in this country, in the 'Annals' for June and December 1863 and March 1864.

The whole of these forms, which, for reasons to be presently given, were referred by me to the genus *Difflugia*, threw an entirely new light on the relations borne by the animal to the shell, or (as it ought to be called in the case of the testaceous Rhizopods) the test *, which the animal inhabits but is only to a certain extent instrumental in constructing. The clue to this most interesting and till then novel fact had revealed itself to me in some of the living organic forms obtained in soundings made in the North Atlantic in 1860 on board H.M.S. 'Bulldog' †, the tubes of certain minute

* It would rid us of a very troublesome source of uncertainty and confusion were the term *shell* confined to the shells of the Foraminifera; *skeleton* or *framework* to the internal siliceous structure of the Polycystina, Acanthodesmidæ, and Dictyochidæ; and *tests* to the more or less chitinoid coverings of the Difflugidæ, Lagynidæ, and allied forms. As it is, these terms are employed indiscriminately and without any definite meaning attaching to each.

† 'The North-Atlantic Sea-bed,' G. C. Wallich, 1862, part 1, pp. 146, 147; and 'Biology of *Globigerina*,' 1876, pp. 11 and 12.

Annelids being invariably found made up of mineral particles, with sponge-spicules and minute Globigerine shells, or a mixture of these in proportion as the mud at the bottom of the ocean, on which the creatures lived, was more or less composed of varying quantities of these materials. This opens out a very important question, which may be expressed as follows:—Is there, or is there not, any connexion in a physiological sense between increased or diminished complexity of structure in the tests of the various testaceous families, and an increased or diminished complexity in the organization of the creatures inhabiting them? For, should the answer be in the negative, the only reasonable inference to be drawn from the facts is that mere differences in the material, mode of building up, and outward form and appearance of the tests, furnish no trustworthy characters for generic or even specific distinction. Or, to take the case of the Foraminifera, it equally becomes a question whether increased complexity in what Dr. Carpenter very appropriately calls "the plan of growth" of the shells can be regarded as indicating coexistent increase or decrease in the complexity of organization of the animal to which the tests belong. In this instance, however, it seems out of our power, in the present state of what ought to be termed our ignorance rather than our knowledge, to furnish any satisfactory answer, inasmuch as no means or methods of observation are available, even with the highest powers of the microscope, which can enable us to resolve those subtle traces of organization, the existence of which we may suspect, but cannot demonstrate. To assert, however, that highly complex functional effects take place in the bodies of these so termed unsurpassably simple creatures, in the absence of any adequate signs of organization, is so absurd that the wonder is that such a proposition should ever have been seriously propounded and unreservedly accepted. In touching on the same question in relation to a very different class of organisms, namely the Desmids and Diatoms, the case was thus stated by me:—"We know that complex vital processes are carried on in even the lowest types of being. But because we neither know nor are able to conceive *how* they are carried on we are not warranted in taking for granted that what appears to us, even with our most refined appliances, to consist of a mere particle of structureless jelly, must necessarily be as primordially simple as it appears"*.

To this opinion I would still adhere: but a voice infinitely

* "Are the Desmids and Diatoms 'Simple Cells'?" G. C. Wallich, 'Popular Science Review,' April 1877, p. 131.

more potent than mine has spoken on the same subject, and in words too pregnant with meaning and truth to be disputed. I allude to Prof. Tyndall, who writes as follows:—"Have the diamond, the amethyst, and the countless other crystals formed in the laboratory of nature and of man no structure? Assuredly they have; but what can the microscope make of it? Absolutely nothing. It cannot be too distinctly borne in mind that between the microscope and the true molecular limit there is room for infinite permutations and combinations. *It is in this region that the poles of the atoms are arranged, that tendency is given to their powers, so that when poles and powers have free action, proper stimuli, and a suitable environment, they determine first the germ and afterwards the complete organism.*"—*Fragments of Science*, London (6th edit.), 1879.

It only remains for me to point out that attention was not invited to the "potentialities" of organization in the sarcodic bodies of the Rhizopoda, with a view of bringing them to bear on the questions we are now engaged in investigating, but solely to show that the existence of these potentialities ought to be recognized, although for the present we must rest content to avail ourselves of such characters as are made palpable to our senses with the aid of the microscope.

This being clearly understood, let me observe that no satisfactory evidence has as yet been discovered of any generic difference between the animal we call *Amœba* and the animal we call a *Difflugia* beyond the palpable one which hinges on the fact of the former being a naked and the latter a testaceous Rhizopod. The sarcode-body in both presents the same degree of differentiation into what is known as endosarc and ectosarc. In both it is provided with a nucleus and contractile vesicle. In both there is a definite anterior and posterior part, the function of the latter being to exercise a certain degree of prehensile action, that is to say to the extent of regulating the movements of the body in the naked forms and maintaining its position within the test in the testaceous ones. In both there occur sarcoblasts, oil-globules, and crystalloids; and in both we may observe extensive vacuolation and the gradual development of a membranous investment of the entire body-substance when encystation is about to take place.

But if outward characters are to be taken at all as our guides, the identity of the two animals in *Amœba* and *Difflugia* can be shown in a still more striking manner. Thus it frequently happens that a Difflugian *Amœba* will vacate its test whilst under observation, and sally forth as a naked *Amœba* without appearing to have sustained any injury or

suffered any inconvenience. And it happens just as commonly that an ordinary naked *Amœba* will, whilst under observation, take summary possession of the first empty Difflugian or Arcellian test that comes in its way, and at once make itself quite at home in its new quarters; the newly-assumed characters being in each instance so perfectly sustained as to leave an observer who has not actually witnessed the transformation no reason to suspect the now testaceous form to have ever been otherwise than testaceous, or the now naked form otherwise than naked.

Other analogies and identities of procedure might be cited, as, for example, those connected with the process termed zygosis, of which nothing is in reality known, though several hypothetical explanations have been hazarded on the subject. So far, then, we encounter no anomaly; but should we push our investigations a step or two further we find ourselves confronted by what at first seems to be not only an unrecognized anomaly, but a paradox. And here *Gromia* retaliates on those who once degraded it, not only by refusing to throw any light on the difficulty, but by doing its best to lend force to it. Formerly, as we now are aware, *Gromia* was wrongly held to be the type of "the very simplest form of Foraminifer," by virtue of the so-termed Reticularian type of its pseudopodia. Yet in recognition of its possessing a nucleus and contractile vesicle, it has been promoted to the highest status in the Rhizopod scale. Its test is one of the simplest to be met with in the highest order, and, when it stood side by side with the simplest Biloculine Miliolidæ in the lowest order, was firmly believed to be just as simple in organization as they. But we have it on the authority of Dr. Carpenter, who probably knows more than any other man living of the structure and "plan of growth" of the shells of the Foraminifera (and any one who has under his guidance studied these exquisitely formed structures must have arrived at the same conclusion), that the Foraminifera, which stand at the very bottom of the Rhizopodal series in point of bodily organization, possess "*shells which are unsurpassed in symmetry and* complexity *of structure by any testaceous organisms*"*.

On the other hand, we see in the highest order of the Rhizopods the animal of *Difflugia* and its now firmly established compeer (as regards complexity of bodily organization) both in possession of protective coverings, the extreme *simplicity* of which is "unsurpassed by that of any other organisms!"

* 'The Study of the Foraminifera,' by Dr. Carpenter, F.R.S., 1862, Preface, p. viii.

In the case of the Difflugidæ there is no anomaly. For, although in the tests of the new forms to which I shall hereafter have occasion to refer in detail, some singularly striking characters become noticeable, there is, strictly speaking, no complexity in their construction *as imparted to them by the animal*, but only a very exceptional character, which carries with it indisputable evidence of not being the result of inherited idiosyncrasy, but of the variable nature of the conditions present in the medium in which the animal lives. This view was strongly urged by me in my paper in the 'Annals' for March 1864, and in a previous paper in the same Journal for Dec. 1863, in the following words:—"At the most, therefore, mere modifications in the shape and proportionate quantities of the organic and inorganic elements entering into the formation of the shell, ought to be employed only in discriminating between species."—*Annals*, June 1863, p. 452. And again:—"Assuming from the facts which have been advanced that the shape, materials, size, and colour of the Difflugian tests furnish characters so conspicuously variable as to yield no trustworthy criterion for even generic or even true specific distinction, and recalling to mind once more that the animal is in every instance specifically the same, it appears to me impossible to arrive at any other conclusion than that the whole of the subspecies, as well as their intermediate varieties (widely though some of these seem to differ from others in *external* features), have not only been derived by direct descent from a single progenitor, but may still continue to be produced by direct descent from varieties which become permanent*; and may one and all still be produced from a common archetype under the varying conditions to which these lower forms of life are subject. The animal does not vary, but it modifies the architecture of its habitation and the mineral material of which that habitation is in a great measure constituted, in obedience to local conditions and its own requirements."—*Annals*, March 1864, p. 239.

* "Permanent" only in the sense of being so as long as the conditions under which the species or variety first became established remain unchanged. When these conditions become gradually or suddenly modified, so do the species or varieties, but only in those respects in which the conditions effect a change in the animal itself, in its shelly covering, or in both combined. Thus, a dry season or a flood, or extreme degrees of temperature in the medium in which the animals live, scarcity or deterioration in the food-supply, one and all bring about modifications which then tell on their stability, their tendency to variation, or their extermination. This, in all probability, is the reason why we so often find some special form we have been accustomed to look for in a given locality, either replaced by a varietal form or gone altogether.

But it would obviously be the height of rashness and an indication of great want of discriminative tact to entertain the idea that what appears to be a rationally grounded explanation in the case just cited, stands on a par with that involved in the construction of all the varied and complex forms of Foraminiferal shell. Here we meet with presumptive evidence of the interposition of some faculty superior in kind to that by which the creature is enabled to select from the materials within its reach those materials best adapted for its requirements. That the Difflugidæ, in like manner with other Protozoa, do possess and are able to exercise some such faculty, is almost as certain as that two and two make four. Several extraordinary oceanic examples of this were recorded by me as long ago as the year 1858, and frequently since that period. But in the Foraminifer there resides not only a like selective power, when the necessity arises for its exercise, as we see in the case of the Lituoline and Arenaceous series generally, when seemingly forced to employ sandy or other particles for the consolidation of their shells on account of the supply of carbonate of lime held in solution in sea-water, falling short; but likewise a constructive faculty of so marvellous a nature as to leave us in a state of utter bewilderment at the beauty and symmetry of construction we see before us. For, be it observed, there is in this instance no tangible basis on which we could attribute what we see to the interference of some known extrinsic force, such as chemical affinity or a modified form of crystallization in presence of a colloid. In this dilemma how are we to account for so truly extraordinary a phenomenon exhibiting itself at the very bottom of the animal series?

On my own behalf I can only confess my utter inability to suggest a solution of the problem.

The inquiry having thus, step by step, reached the point at which any special group of characters observable in the testaceous Rhizopods under notice can be tested on the basis laid down in the opening paragraph of this paper, let us now turn our attention to Prof. Leidy's monograph on "The Freshwater Rhizopods of North America," the most recent and by far the most beautifully illustrated work on the subject that has hitherto been published *.

The first point deserving of notice is that Prof. Leidy does not offer any definite classification of his own of the freshwater Rhizopods, but confines himself to furnishing a more or less general outline of classification of the various systems proposed by Dujardin, Hæckel, Carpenter, Wallich, Huxley,

* Published at Washington in 1879, under the auspices of the "United States Geological and Geographical Survey of the Territories."

Carter, Hertwig, Greef, and others. Indeed, as he himself admits, "his attention has been more particularly directed to the discovery and determination of the various forms of Rhizopods occurring in North America, rather than to the elaboration of details of structure, habits, modes of development, and other matters pertaining to their history, though these have not been entirely neglected" (*op. cit.* p. 2).

The only portion of the volume that appears to me to fall short of the general standard of technical excellence is the purely bibliographical index, which is here and there rendered almost unintelligible through an undue multiplication of synonyms and the clerical errors which have occasionally crept into it. But its very compendiousness, which of itself must have involved a vast amount of labour, may well be allowed to turn the balance against any shortcomings of the kind referred to.

I sincerely wish certain errors in the work, of another kind, could be as easily passed by without further comment. Unfortunately, for reasons which will develop themselves as I proceed, they cannot be so. But when they are pointed out, I venture to think that, from whatever cause they may have arisen, Prof. Leidy himself will be the first to acknowledge them, quite as much in his own interests as in mine.

Nothing, therefore, of minor import to me personally than the facts about to be noticed could have induced me to criticise certain statements made by Prof. Leidy in reference to my published opinions concerning the freshwater Rhizopods, in a manner which, although unavoidably adverse, will, I trust, never appear hostile; more particularly as the United States Survey Department have done me the great honour of presenting me with a copy of his magnificent volume.

At page 7 Prof. Leidy makes the following remark:—"Dr. Wallich (Annals & Mag. Nat. Hist. 1863, xi. p. 438) divides the Rhizopods into three orders, the Herpnemata, Protodermata, and Proteina. In the first are included the Gromidæ, Foraminifera, and Polycystina; in the second the Thalassicollina and Acanthometrina; and in the third, the Actinophryna, Lagynida, and Amœbida."

As already stated, within a couple of years after the issue of the June 1863 number of the 'Annals' from which the above paragraph was taken, it was proved by me, not, as Prof. Leidy observes at p. 279 of his work, "in one instance," but in a sufficiently large number of instances to place the point at issue beyond dispute, that *Gromia* normally possesses both a nucleus and contractile vesicle, and must therefore, in spite of its "reticularian" pseudopodia, be

transferred from the lowest to the highest order of the Rhizopods. Had Prof. Leidy read the observations made by me at a somewhat later period (to which attention has been already drawn at pp. 322, 323, *ante*), he would have seen that, for the important reasons assigned, *Gromia* had been so transferred, and would, in all probability, therefore have accorded the fact as prominent notice as he accorded the statement contained in the paragraph above quoted. But he made the matter worse by stating at p. 279 of his work, without any further explanation, that—

"Prof. Schultze intimates the absence of a contractile vesicle in *Gromia* (Arch. f. mikrosk. Anat. 1875, p. 116); but *Dr. Wallich remarks that in one instance he detected this* temporary (!) *organ in Gromia oviformis*:" the most unintelligible part of the affair being that he should have stopped short in his quotation of my paper at the very point where my reasons were given for not deeming it expedient to speak positively about the presence of the contractile vesicle in *Gromia* on the strength of a single observation, and consequently determining to await its confirmation through a sufficient number of further observations.

I repeat, had Prof. Leidy cited the whole of the passage referred to, he might have been induced to consult two of my later papers, namely one on "The Affinities of the Polycystina" (mentioned in his Bibliographical list under my name), which was published in the 'Quart. Journ. Microsc. Science' for July 1865, and another "On the Fundamental Error of constituting *Gromia* the type of Foraminiferal Structure," published in the 'Annals' for Feb. 1877, and have thus avoided so obvious a misapprehension of my observations, and one so calculated to throw unmerited discredit on the entire basis of my classification.

But so completely did Prof. Leidy misinterpret or overlook my writings in relation to *Gromia*, that at p. 277 he expresses himself as follows, under the head of "Foraminifera:"—"These, though consituting the most extensive and important order of the Rhizopods, are almost exclusively marine. A single well-known genus, *Gromia*, is represented by several species inhabiting salt and fresh water;" and in the page following the last named, "The genus is of special interest *because it is a representative, in the simplest condition, of that great order of Rhizopods, the Foraminifera,* which are exclusively marine with the exception of the present one, *Gromia*." And at pp. 278–279 he says that the body of *Gromia* "contained a large clear or pale granular nucleus situated centrically or eccentrically, and also variable proportions of

vacuoles. . . . . A vacuole was at times observed to gradually disappear; . . . . . but it was doubtful whether any of these corresponded with the contractile vesicle of other Rhizopods." He then gives a very good description of the characters of the only form of *Gromia* he had met with in North America, named by him *G. terricola*, partly on account of its habitat "in the crevices of the pavement in the yard attached to his home in the city of Philadelphia," and partly, I presume, owing to the animal having a habit of accumulating at the posterior portion of its test "more or less dirt consisting of fine granules and coarse particles of quartz sand" (p. 280). But beyond this his description of *G. terricola* would hold just as good for *G. oviformis*, or indeed any of the polymorphous varieties assumed by these organisms, for it presents no new characters.

The second erroneous statement I have to notice is even more extraordinary than the former one, inasmuch as it does not involve a misapprehension of my written opinions, but attributes to me statements which are directly opposed to those really made by me on the points in question. I allude to Prof. Leidy's assertion in relation to *Difflugia symmetrica* and the entire series of new testaceous forms, of which, with three before-mentioned exceptions, not one had been previously discovered, so far as I am aware, either in this country or elsewhere, prior to the appearance of my paper "On the Extent and some of the principal Causes of Structural Variation among the Difflugian Rhizopods," published in the 'Annals' for March 1864.

At pp. 150 and 151 of his work Prof. Leidy says, "The series of specimens represented by Dr. Wallich in figs. 27 to 33, pl. xvi. of the 13th vol. 'Annals & Mag. Nat. History' for 1864, and *described as transition forms of Difflugia symmetrica*, appear to me to pertain to the same animal as *Nebela collaris*."

It is not for me to hazard a conjecture how such a distorted view of my clearly-expressed opinion regarding the transitional series of forms referred to could have been arrived at by so careful an observer. At all events, I can positively affirm that I never entertained or expressed such an opinion. In all I wrote on the new varieties of the Difflugidæ I referred only to the outwardly visible characters of the tests for reasons already stated; and neither directly nor indirectly described "the *specimens* represented in my figures 27 to 33 of pl. xvi.," as "transition forms of *Difflugia symmetrica*." What I did state was that I considered them all as varieties of *Difflugia proteiformis* or its variety *D. pyriformis*; and as such I must continue to regard them until some much more satisfac-

tory reasons for cancelling my title to priority and superseding the generic position to which I referred them shall have been produced than those offered in Prof. Leidy's volume.

In my observations on the Difflugian Rhizopods, in the 'Annals' for March 1864, above referred to, I endeavoured to show that the entire series of Difflugian tests represented in my plates are constructed by animals which, with no known exception, are generically as well as specifically identical. There is nothing improbable therefore in the assumption that the entire series in their *earliest* condition, that is to say when the chitinoid exudation of which the test is entirely composed makes its appearance around the sarcoblast, are identical in form. When we study forms obtained from a sufficiently wide geographical area we find many previously existing intervals between varieties bridged over; and if we note the differences in the external conditions by which the animals are surrounded, whether of locality or climate, we are able, generally speaking, to trace some relation between the peculiarities of the varietal forms and the physical agencies which have helped to produce them. But in the cases under notice, neither in the structure nor the degree of organization of the animal itself, nor in the outward figure of any of the forms of test, are there any differences to be detected which could distinguish them generically from their exact prototypes and counterparts in already well-known and established typical Difflugian forms. For, as I have always maintained, the changes brought about in the external characters observable in the tests of the new varieties described by me in the 'Annals' for March 1864, are purely dependent on contact of the chitinoid bases of the tests with materials present in the medium by which they are surrounded, and therefore ought not to be employed for generic or specific subdivision.

A great deal of additional evidence in the same direction might be now adduced from my previous writings did space allow. Before proceeding further I must therefore confine myself to offering a few brief remarks bearing directly on what has gone before.

Without the production of any satisfactory reasons for his statements or for taking such a step as giving a new generic name to *Difflugia symmetrica*, which, as he himself admits, had been first described by me, Prof. Leidy thus defines the new genus he has created under the name of 'QUADRULA:' —"Shell compressed pyriform, transparent, colourless, composed of square plates of chitinoid membrane arranged in transverse or more or less oblique series, in consecutive or

alternating order. Mouth inferior, terminal, oval. SARCODE COLOURLESS, HAVING CHARACTERS OF THAT OF *DIFFLUGIA*, &c." (*op. cit.* p. 142).

In describing the *species* he says:—"*Quadrula symmetrica*, the only representative of its genus, is remarkable for the peculiar construction of its shell, which is compressed *pyriform*. . . . . The general arrangement [of the plates] is like that of tiling with variable regularity. . . . They are not entirely disposed with the symmetry expressed by their name, for frequently smaller plates break the regular succession of larger ones, and sometimes one angle of a plate replaces that of a contiguous one" (*op. cit.* p. 143). And, again, "*Quadrula symmetrica* was first described" in 1863–64 "by Dr. Wallich, *under the name* of *Difflugia symmetrica*, from specimens found in England. It was more recently" (that is to say in 1875, or just eleven years after I described and figured it) "described, and referred to a new genus, by Prof. Schultze from specimens found near Dresden. Ehrenberg described the same as pertaining to three different species under the names of *Difflugia assulata*, *D. carolinensis*, and *D. leptolepis*. These, in 1871 (Abhandl. Akad. Wiss. Berlin, 1871, p. 246), with a number of other forms, he referred to a subdivision of *Difflugia* with the names of *Assulina* and *Hologlypha*. As, however, the latter would apply to the first members of the subdivision indicated, which appear to be *only varieties*, or at most two species of *Cyphoderia*, neither of the names could be considered as appropriately taking precedence of *Quadrula*, distinctly applied to *Assulina assulata*, the fourth member of Ehrenberg's list" (of 1871).

As a matter of fact, *Difflugia symmetrica* is *the only* aberrant member of my series of new testaceous Difflugidæ which was *not* included in the synoptical list given at p. 240 of the 'Annals' for March 1864, being then, as it is still, considered by me to have been sufficiently identified and defined in any classification having for its end a systematic arrangement based only on natural characters. Moreover, it seems extraordinary that the established rules of priority and nomenclature (to which Prof. Leidy here draws such marked attention) should, with his sanction, have been infringed by Prof. Schultze, when the latter writer, in 1875, superseded the generic name given to the form in question, at the same time retaining the specific name applied to it by me as distinctly indicative of its special character.

I venture to assert there is not a single new character assigned in Prof. Leidy's definition of the genus "*Quadrula*" (or, to use an expression of his, *Difflugia symmetrica* "under

the name of" "*Quadrula symmetrica*") beyond those adduced by me, except one which I undertake to say is erroneous, namely that "the plates are either chitinoid or membranous." On the other hand, he undoubtedly furnishes the completest proof of the propriety of referring the form to the genus *Difflugia* when the only remark he has to make upon the animal is that the sarcode "has the same character as that of *Difflugia*." This ought to be borne clearly in mind, for Prof. Leidy subsequently speaks of *Quadrula symmetrica* as "the only representative of its genus."

But it is quite needless to argue the question of priority a step further, for I now have to place on record an important fact of which I was ignorant at the time I described *Difflugia symmetrica* in 1863–64, and discovered only within the present year, viz. that this identical form had been figured in Ehrenberg's 'Infusionsthierchen' as *a Difflugia*. Ehrenberg's subsequent remarks in the 'Proceedings of the Berlin Academy' and elsewhere, to which allusion is now made by Prof. Leidy, are altogether beside the question at issue, except to the extent of proving that Ehrenberg recognized the validity of my specific appellation of "*symmetrica*" and retained it. Of course, the moment I found I had overlooked Ehrenberg's title to priority (unfortunately too late to be made known to the illustrious dead), I determined on the first suitable occasion to cede all title to the discovery of *D. symmetrica*, though I was undoubtedly the first to detect it in this country, and to discover, describe, and figure the other new forms of testaceous Difflugidæ of which I shall have occasion to speak in the concluding part of this paper.

[To be continued.]

XXXI.—*On a Collection of Lepidoptera made at Manipur and on the Borders of Assam by Dr. George Watt.* By Arthur G. Butler, F.L.S., F.Z.S., &c.

[Plate VIII.]

[Concluded from page 310.]

Lycænidæ.

69. *Cyaniris placida.*

Cyaniris placida, Moore, P. Z. S. 1883, p. 523, pl. xlviii. fig. 5.

Near Assam.

Only males of *C. placida* were obtained.